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THE PRESENT ASPECT OF SOME  
CYTOLOGICAL PROBLEMS.

*The Cell in Development and Inheritance.* By E. B. Wilson, Ph.D., Professor of Zoology, Columbia University. Second edition, revised and enlarged. Pp. xxi + 483. (New York: The Macmillan Company. London: Macmillan and Co., Ltd., 1900.) Price 14s. net.

DURING the few years which have elapsed since the appearance of the first edition of Prof. Wilson's book on the cell, the rapid accumulation of new facts has resulted in the modification, in many important respects, of the views which were entertained concerning many cell phenomena even so lately as five years ago. Hence, though the volume before us is issued as a second edition, it has not only been considerably enlarged, but also much of the original matter has been displaced to make room for new treatment which shall more faithfully reflect the attitude of cytologists towards the problems which confront them at the present time. And we may fairly say that the author's efforts have not only been largely successful, but they have resulted in the production of one of the best works which it has been our good fortune to meet with for a long time.

The whole subject is handled in an easy and masterly fashion, and the reader is enabled readily to grasp the leading facts and to obtain a clear insight into the nature of the chief questions of cytological importance. Of course the book is not without its faults, but they are not, for the most part, serious ones. The author is naturally less at home when dealing with botanical than with zoological work, and indeed he says as much in his preface; but we notice, here and there, slips which might have been easily avoided. Amongst the most obvious of these is a somewhat misleading account of the morphology of the embryo-sac on pp. 264-5. A more trifling matter is the rather irritating recurrence of eulogistic adjectives, and the reader is apt to weary of a "brilliant" hypothesis or an "interesting" observation which is shown a few lines farther on to be untenable or unsound.

The volume opens with a brief historical introduction, in which it is satisfactory to find that Cohn is given the credit, which undoubtedly belongs to him, of having been the first clearly to identify sarcode with protoplasm. Then follow chapters on the division of the cell and the nucleus, and of the more intimate structure of the cell-constituents. In the account of the details of karyokinesis, a fairly representative series of examples is given; but we could have been well contented had the author seen fit to amplify his treatment of the simpler and more primitive forms of life, seeing that so many of them exhibit remarkable and suggestive deviations from the course of events as pursued in the higher animals and plants.

Prof. Wilson deals with the vexed questions which have arisen concerning the centrosome in a cautious and

discriminating manner, and he discusses the various theories which have been put forward respecting the nature and functions of this highly enigmatical body. A considerable mass of evidence has been gradually accumulating which tends to show that a greatly exaggerated importance has been assigned to it by many investigators. There are instances in which it can only be recognised as a transitory structure which persists during special phases of activity, to disappear when these subside. The view was at one time current amongst the majority of cytologists that the centrosome represented a permanent structure which presided over the divisions of the nucleus, and that it was, in fact, *par excellence* the organ which aroused and directed the karyokinetic processes. More extended investigation has, however, failed to support this proposition, and in not a few cases, especially amongst the higher plants, there is no good evidence of the existence of a centrosome at all. Furthermore, the researches of Hertwig, Morgan, and especially the recent ones of Loeb, have proved that eggs in which the original centrosomes have undergone complete degeneration are yet capable of exhibiting the entire processes of division when appropriately stimulated, and this without the entrance of a sperm or any other centrosome-bearer whatsoever.

The question as to the permanence of the chromosomes is also considered, and, on the whole, Prof. Wilson appears to incline to the view that the *same* chromosomes which were visible in the daughter nuclei at the close of a division reappear when the latter proceed to divide once more. In conformity with this idea, he supports the hypothesis that in cases where less than the normal number of chromosomes arise in a nucleus, these are in reality plurivalent—that is, each apparent chromosome is compound, and represents two (or more) true chromosomes united together, although their individuality may be for the time entirely masked. Thus it is well known that the nuclei of both the ovum and the spermatozoon possess only half the number of chromosomes characteristic of the somatic cells of the organism, and that this "reduction" is accomplished in connection with two peculiar and rapidly-succeeding nuclear divisions. Each of these chromosomes (at least in the first division) is then regarded as plurivalent (bivalent)—that is, as composed of, at any rate, two individuals which have not separated from each other. One necessary consequence of this view is that somewhere during one of these divisions, or at any rate before the formation of the sexual cells which arise from them, there must be a *qualitative* distribution of the real primary chromosome-individuals between two nuclei. Such an occurrence was regarded as antecedently probable by Weismann, and his views received a remarkable confirmation at the hands of several investigators, who describe the sequence of events as proceeding in a manner such as to render it apparently clear that a *qualitative* distribution does actually occur.

On the other hand, many others have been unable to find any evidence for the existence of such a type of division in other organisms, and conclude that the facts are strongly opposed to it. Should their view be correct, even in the case of a single example, the whole objective

evidence relied on by those who see in the phenomenon of chromosome-reduction a confirmation of Weismann's theory falls to the ground. And with it, also, the hypothesis of plurilinquency and continued persistence of the chromosomes suffers a serious limitation, for it is obvious that a time must soon arrive, in the sequence of generations, at which the evident chromosomes themselves can no longer consist of the telescoped chromosome units of all the previous life-cycles. In short, each chromosome that appears after the reduction in number cannot be represented in terms of the somatic units as  $a+b$ , but it must possess a new structure  $c$ .

Prof. Wilson very fairly reviews the evidence for and against a *qualitative* reduction (of the quantitative or numerical reduction there is, of course, no question), leaning, as has been said, somewhat in its favour, and it must be admitted that there is some indirect evidence in support of it. Perhaps it hardly falls within the scope of the author's work, but a consideration of the reversion of hybrids to the original stocks, such as indicated by Mendel's law, which has recently formed the subject of important communications by De Vries and by Correns, might have been discussed in this connection.

Exigencies of space forbid us to do more than to indicate the excellence of the treatment of the structure and development of the spermatozoon, of the phenomena of fertilisation, and of parthenogenesis. Our views as to the essential nature of fertilisation are undergoing a change in certain respects as the result of cytological investigations in this field of inquiry. We have clearly to recognise the existence of two distinct factors in the process. The one is concerned with the stimulation of the egg, which is thereby impelled to segment and to develop into a new organism, the other is involved in the fusion of the two sexual nuclei.

Boveri's experiments long ago showed that a fusion of the male and female nuclei was not essential to the segmentation and organised development of an egg. He succeeded in fertilising non-nucleated fragments of echinoderm eggs with the sperms of another species, with the result that larvæ exhibiting the paternal characters only were formed. These experiments were for some time regarded as not being free from objection, but they have been repeated with similar results. Again, as Loeb has recently shown, it is possible, by treating the unfertilised eggs of *Arbacia* with a solution of magnesium chloride, to cause them when replaced in sea-water to give rise to normal larvæ. And once more, Nathansohn has proved that, in the case of *Marsilea*, a sufficiently high temperature suffices to excite parthenogenetic development in the oospheres of these plants. Even in many normally fertilised eggs it has been repeatedly shown that the stimulus which starts the karyokinetic processes in the egg comes from the cytoplasmic (centrosome) portions of the sperm rather than from its nucleus.

As regards the significance of the nuclear fusion, although we are as yet unable to speak with certainty as to its proximate or efficient cause, there can be little doubt but that its teleological significance is to be sought in the fact that these bodies contain in themselves the physical basis of heredity, and thus by their coalescence the hereditary qualities of both parents are mingled in the offspring.

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#### METHOD IN PHILOSOPHY.

*Die Transzendente und die Psychologische Methode.*

Dr. Max F. Scheler. Pp. 181. (Leipzig: Dürr'schen Buchhandlung, 1900.) Mk. 4.

IN opposition to the positivism which avers that if we take care of facts method may be left to take care of itself, Dr. Scheler claims that the history of thought, its continuity notwithstanding, shows abundantly that each fresh conquest in knowledge is preceded by a definite, if often half-conscious, breach with outworn method. Kant's historic mission has been fulfilled, and, after a century's probation, the time has arrived to pass beyond him. Not, however, by the adoption in philosophy of that psychological method which, discredited in Condillac and Hume, has been encouraged by recent advances in technical psychology to essay rehabilitation in more plausible forms. Dr. Scheler is with contemporary psychology in its reaction in favour of real as against formal principles, development as against finality, the historical as against the mathematical temper. He is with the Kantian in his recognition of the *quaestio juris* and in his advocacy of an inverse or "reductive" method. In the result he accepts a formula from Eucken—that of a regress from "the well-founded phenomenon" of a culture embodied in a coherent aggregate of institutions to the real forces of which it is the living and still growing product. *Arbeitswelt* and *Geistige Lebensform* are the catchwords of this "noölogical" method.

Dr. Scheler's discussion of "transcendental" method, *i.e.*, the inference from an accepted group of facts to the principle which can and can alone explain them—what evidence of the "alone" could be adduced it is hard to see—directs its main attack, not against its inverse character, but against (1) its static nature due to acceptance of an immutable starting-point, (2) its formalism in its conclusion to grounds of a merely logical kind, (3) its intellectualism with its consequent neglect of "three-fourths of life." Its alleged "synthetic propositions *a priori*," *i.e.*, propositions at once instructive and necessary, are really experiential. The starting-point is really dependent on psychology. Change the period and get a different psychological "climate," and you will find that the transcendental presuppositions will be different. But if so, the psychological ground-propositions will be complete in themselves, and formal conditions established by transcendental deduction are superfluous. The only regress which is not simply a doubling of the data must be towards real, that is, actual and active principles. And the data are neither unchanging nor purely rational.

This general appreciation Dr. Scheler reinforces by a detailed treatment of space, time, causation, and personality. As regards space, the temptation to strengthen the charge of formalism by putting Kant out of touch with a perceptual world has proved too strong for Dr. Scheler. Kant's "empty" space probably means only that all particular contents of space can severally be thought away without altering our space-apprehension. Kant's space is voidable rather than void. Geometer's space, while it is in one sense an abstraction, is not only not a generic concept, but not a concept at all, if the argument as to whole and parts in Kant's metaphysical exposition is to stand. The psychogenetic problem of the perception of a third dimension is irrelevant to Kant's nativism.